

Squibb (E. R.)

On the Relations of the Medical Profession
to the Trade Interests of the
Materia Medica,

AND

A NOTE ON PEPSIN,

BY EDWARD R. SQUIBB, M. D.,

OF BROOKLYN,

FOLLOWED BY A RE-PRINT OF A PAPER ENTITLED

A READY METHOD OF TESTING PEPSIN,

RE-PUBLISHED FROM

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McClure

NOTE.

The following papers are republished from THE PROCEEDINGS OF THE MEDICAL SOCIETY OF THE COUNTY OF KINGS for May, 1880, where they are given with a report of the discussions elicited by them.

When read before the Society at the April meeting, they caused much discussion, and parts of the first paper, which was written for presentation at the March meeting, were strongly objected to.

Therefore, as the grounds taken in the paper were so strongly opposed as to give occasion for a special meeting of the Society, to consider whether or not the paper should be published;—and as the Society ordered the publication of the paper criticised, on “A Ready Method of Testing Pepsin;” —and as the method of this paper was supported by some members against the evidences of its fallacy presented, it seems but fair to both sides of the question to reproduce that paper here in direct connection with the criticisms made upon it.

The conclusions of both these papers may be erroneous, and those of one of them must be so. But if one paper be more inaccurate and untrustworthy than the other, the support which it received certainly shows the great danger of accepting the testimony of papers as proving that the statements set forth in them are facts.

Brooklyn, May, 1880.

E. R. SQUIBB.



ON THE RELATIONS OF THE MEDICAL PROFESSION TO THE TRADE INTERESTS OF THE MATERIA MEDICA.

BY E. R. SQUIBB, M.D.

It is hazardous to the best interests of both the science and the art of medicine for medical men, either individually or through their organizations, to publish anything that can be used to advertise individual or proprietary specialties for business purposes.

The medical journals of the day owe their existence as such very largely to the pecuniary support obtained from the advertisements upon their fly leaves; and when these advertisements can be made to pay the advertisers better by the use of names or societies, or by references to published papers, such uses and references are very likely to be made.

Judging by their actions, a very considerable proportion of the medical profession consider such uses of their names and their papers entirely legitimate. Another proportion give their names and write their papers without thought as to whether they can or will be so used, or not; while another small proportion appear to give their names and write their papers especially for advertising purposes.

Those whose names and papers are so used become at once special partners in business enterprises, or endorsers of speculative business paper; and if this fact could but be recognized by all readers of such names, papers and advertisements, the risk of harm would be much less. But, unfortunately, the relation is not generally recognized in the medical profession, though very promptly recognized and utilized by the mercantile or trade interest involved. Hence, when the medical man joins the business man in extending the profits of his business, the medical man necessarily joins in the risks and liabilities of the business, and loses in reputation and professional tone and dignity all that the business man gains by him, while the business man gets all the money. What the medical man expects and hopes to gain, by lending his name and influence, is a good specialty for the use and benefit of his profession at large. And in his hope and through his enthusiasm he disregards the experience of both past and present time, throughout which fallacies and quackeries have passed and are passing into disuse, despite the physicians' certificates and endorsements, and so-called experience in their use, dragging down with them the names of their endorsers. The business man, however, does not go down with his wares and his doctors' names, but having harvested his profits, invests a part of them in new enterprises.

It was wonderful that Swaim's Panacea, Cundurango and Missisquoi water could each in turn repeat the history of its predecessors, and that hundreds of other articles could keep the line of succession unbroken; but it is still more wonderful that upon the testimony and by the support of the medical profession, through its advertising tendencies, the number of such specialties constantly increases. The endorsed statements of to-day in regard to the various foods for infants—Pepsins, Chloro-pepsins, Lactopeptin, Ingluvin and Pancreatin—if accepted, and if only enough of them were bought, would leave the condition of indigestion out of the list of human ills. Add to the perfect digestion and assimilation thus insured to young and old the endorsements of the tonic series of Beef, Wine and Iron, and the Elixirs, and there is hardly anything left to wish for except the endorsements of Damiana. Bladder-wrack or Anti-fat might indeed be needed. But how there should be any consumption left for Benzoate of Sodium to cure, or any of the long list for Pond's Extract and Radway's Ready Relief, Holman's Liver Pad, etc., cannot be logically understood. Yet it is not at all doubtful that all this long line of specialties and quackeries owes its unbroken succession, and the proprietors owe much of their money to the advertising and the support obtained from the medical profession.

Let any one interested in this matter—and what physician is not?—glance over the testimonials wrapped round the sample bottles left by the ubiquitous and irrepressible drummer, without thinking so much of the substance endorsed as of the validity of human testimony as evidence; and by the time he has glanced over two or three rival circulars he must conclude that the relation between statements and facts is not materially altered by such testimony,—that florid assertions are mere assertions still, and are yet as far from being established truth as if not supported by prominent names in the medical profession.

Some look to a higher standard of medical education to correct this evil. But if such will glance again at such testimonials, they will find the names of many men whose qualifications, and whose standard of medical education, are far above any general standard that can ever be reached,—names of leaders and teachers high in the profession, and subscribed to patented specialties under such statements as "The most important remedial agent ever presented to the profession."* A higher standard of medical education is undoubtedly needed, and this will help to correct the practice by teaching the value and the character of accuracy in research before statements are made. But is it not rather a higher standard of moral responsibility—of respect for the truth—that is needed

*See cover of "The Practitioner—A Journal of Therapeutics and Public Health," for February, 1880.

here? The statements themselves, in the abstract letter, may be truthful—at least in the sense of being believed by those who make them,—and yet the purposes with which they are given, and the uses to which they are put, make them generally untruthful and hazardous.

Leaving out of consideration that small proportion of the medical profession which purposely lends itself outright to the making and advertising of specialties as a business, the main cause of the evil is, perhaps, that physicians will accept any testimony as evidence, and thus allow themselves to act upon insufficient grounds. For example, if a physician testifies to a special make of pepsin, there are several prominent reasons why his testimony may not be safe as evidence. First, if his testimony be definite and florid enough to be of much value to the manufacturer, it is almost impossible that it should be accurate enough to be accepted as evidence. He cannot be sure that the sample tested accurately represents the whole product of the maker. He cannot be sure of his methods and conditions of testing, without a carefulness and thoroughness of research and observation which is rarely attempted in such cases, because rarely considered necessary, and finally, he cannot be sure that the article, at his time of testing or using, is of the same quality as it will be in the future, when his testimony and endorsement is used for it. The testimony remains unchanged; but, admitting it to be accurately true and just, the substance endorsed may change indefinitely. Other reasons why certificates and endorsements of specialties in the *materia medica* are dangerous may be seen from the following example:

On the notice of the December meeting of this Society, it was announced that a member would read a paper on “A Ready Method of Testing Pepsin.” When the paper was called for at the meeting the author was not present, but the paper was offered and read by another member, who is a prominent manufacturer of pepsin. The test described in the paper is made by precipitating in a test tube one inch wide, the pepsin from ten grains of the sample, dissolved in “about one ounce of water,” by means of an equal volume of a saturated solution of common salt, allowing the whole to stand 24 hours, and then measuring the thickness of the precipitate floating on top, and counting the thickness in sixteenths of an inch.

In the reading of the paper* it appeared that the pepsin of sixteen of the more prominent manufacturers, including that of the reader of the paper, had been submitted to this test many times, and that the pepsin of the reader of the paper was, on an average, more than ten times better than that of the other makers. The names of all the sixteen makers were given, and a table and wood-cut were shown comparing the makers, and with the names attached. In the table, which showed the results of the testings, about eleven of the pepsins had been tested eleven times

* Reprinted at page 16 of this pamphlet.

from the same sample, and, as a rule, about three or four only of the eleven testings of the same sample agreed. In many of the pepsins the same sample, as judged by the test, gave results, in the eleven trials, differing by one hundred to three hundred per cent. between the extremes. The test gave the most uniform as well as the best results with the sample made by the reader of the paper; but even here it varied between 20 and 38, or 90 per cent. The same samples of some makers gave negative or zero results in three or four of the eleven applications of the test, while in other applications the same samples gave the figures 2 and 3. Thus the same sample by the same test showed no pepsin at one trial and three-sixteenths of an inch at another trial. It was explained, however, by the reader, but not in the paper, that when the precipitate was very small it was marked zero.

In the discussion which followed the paper, a member objected that precipitates in general, and especially those which floated at the surface of the mother liquor, varied so much in density that their volume afforded no accurate indication of quantity, and that while the author presented his test as "a qualitative and not a quantitative test," yet that the table and the wood-cut which formed a part of the paper were, in the main, and in evident intention, quantitative, and that upon their quantitative value the chief discrimination between the different makers was based. The substance of the objection was that a test which could only be qualitative, at best, was used in estimating quantity, and upon its quantitative misuse, untrustworthy discriminations were made for and against individuals whose names were used. The reader of the paper then stated that he had a series of results obtained long before this paper was written, showing that the weights of these floating precipitates were in proportion to their volume.

Throughout the discussion, despite the objections, the results seemed to be generally accepted as established facts very important to be known and published, not only to let physicians know where to get good pepsin, but also to know the fraudulent practices of the makers, whose names are given.

The paper then went, in its regular order, to the Committee of Publication, where objection to its publication was made on the ground that the injurious discrimination made against respectable manufacturers was so damaging to their business interests as to render the Society liable to prosecution for damages by the publication of the names used. The author of the paper was then asked by the Committee to make the paper impersonal, by giving results without names. This he declined to do, and, although the paper was put in type, the Committee thought it best to exclude it from the Journal. Before finally excluding it, however, the Committee submitted its action to the Council of the Society, and the action was affirmed by the Council.

As the paper failed to appear in the Journal, and these circumstances became known, members who felt aggrieved by the action of the Committee and the Council brought the matter before the Society at the February meeting, and the liability of the Society to prosecution on such grounds was discussed with much difference of opinion, and with the apparent object of getting the Society to overrule the action of its Council and order the publication of the paper. The result of this discussion was the appointment of a Committee to ascertain the legal responsibilities of the Society in the premises.

It was stated that there were other considerations involved beside the liability to prosecution, and that the Council might have had other objections to the publication. But the Committee raised was restricted to the one point of legal responsibility, so that when its report is presented, any other points to be considered must come from other sources than the Committee. That there *are* other points for consideration, whether the Committee of Publication noticed them or not, appeared during the discussion. One such point was the liability of such a paper to be used as an advertisement by the maker whose pepsin appeared in it to be the only one fit for use. In simply publishing the paper, the Society would not, of course, endorse its conclusions. But it would give it the weight and publicity of its Journal, without which it becomes far less effective. If published, it becomes a most valuable business advertisement, and might be referred to in other advertisements and circulars, by date and page of the Journal, as is customary in such cases. But if not published, such use of the Society could not be made. Such publications may be right and proper if the testing and its results are surely scientific facts accurately stated. But the Committee of Publication may have seen that the results of a test which disagreed among themselves, to an extent varying from 90 to 300 per cent., could not be scientific facts accurately stated; but could only be useful for loose advertising purposes. And the Committee may have seen that an untested test, presented as a qualitative test only, had been taken entirely out of its professed scope and character, and had been made to measure quantities which were represented numerically and set forth in a plausible looking table; and that the results thus stated could not possibly be even moderately accurate, and probably were wholly fallacious.

But the Committee of Publication could not then have known what has since very much strengthened the inference that the paper might be used for advertising purposes, namely, that it has been so used, with a slight modification of the wood-cut of the paper, on the advertising leaves of a medical journal, by the manufacturer of the lauded pepsin who read the paper before the Society, but without using the paper or its author's name, and, of course, without being able to quote

from the Society's publications, as he might have done had it been published. He was present at the February meeting, and took part in the discussion. Had he then stated that he had thus used this test as his own in a published advertisement, giving the names of parties damaged by it, he would have relieved the Society from its interest in the *legal* responsibilities involved, and would have saved the raising of the Committee, because, by this publication of the test and the names, he assumed the liability to prosecution himself.

It was stated, in the discussion, that liability to prosecution could not be great, because in similar cases no prosecution had followed; and an instance was given where the Onondaga County Medical Society had, under similar circumstances, gone so far as to pass resolutions supporting one pepsin maker, and condemning others, without having been prosecuted. But it was also shown, on the other side of the question, that this action of the Onondaga County Society had been spread all over the country as an advertisement, by the makers of the lauded pepsin; and that the pepsin so lauded by *that* Society was among those now sought to be condemned in *this* Society; and, farther, that the pepsin lauded in this paper as about four times better than the Onondaga Society's choice was among those condemned by the Onondaga Society. Such competitive and opposite action in County Medical Societies in supporting the rival claims of manufacturers, can hardly tend to improve the *materia medica* or elevate the standing of the medical profession.

During the discussion, a member spoke warmly to the point of this Society's declining to publish a scientific test given by such authorities as Stillé and Maisch, and announced that he was preparing a paper on this same subject. Another member stated that no such test was given by Stillé and Maisch, but only the process of manufacturing pepsin upon which this test was based. Although both these speakers are engaged in scientific research upon the special article on which they spoke, the scientific accuracy of one or the other *must* be doubtful.

A great deal was said about the scientific character of the paper and the discussion, and of the immunity of all such from processes of law. But both the paper and the discussion were really as loose and as unscientific as could well be. The older, more accurate and better known digestive test must be considered the standard test for pepsin, because the article is of the nature of a ferment, and is used for digestive purposes, and the quantity required to produce a given digestive effect is mainly the measure of the organic matter which protects and preserves the ferment. Although this digestive test is as easy, more accurate, done in one-fourth the time, and with no more apparatus nor manipulation, and, therefore, better and more "ready" than that now proposed—no comparison of the two was given,—no check of the known test against the

unknown, by which precipitation of organic matter holding the ferment as a vehicle could be judged by actual digestion.

This writer was not a member of the Council, nor of its Committee of Publication, which took action upon the paper, and can only rationally infer that they may have had some such reasons for declining to publish it. If the Council feared that it might be used as an advertisement, possibly on the cover pages of its own Journal, then its fears have been realized, for it has been so used, but on the cover pages of another journal, though without the reference to the paper which might have been made had it been published. If the Council feared that the application of an untested qualitative test to the estimation and comparison of quantities and effects was too uncertain and inaccurate to form a proper basis for charges of fraud upon a number of individuals, whose names were given, the Council's hesitation until the names should be withdrawn from the paper, and when this was refused, its refusal to publish was wise and equitable. For if processes of law do not reach the damage done by injurious statements, made on loose and insufficient grounds, and if the law be powerless to require that damaging statements, published as facts, shall be proven, or else the damage done be compensated, this does not alter the justice and equity of the acts. Hence the moral sense and the ethics of this Society should make it avoid doubtful acts, though they be not within reach of penal law, for it may certainly be said that those in its membership

"Who have no check but human law
Are to the few restricted."

It is so much easier to state an opinion as a fact, than to establish a fact, and it is so much easier to establish a fact than to disprove a fallacy which has been stated as a fact, that no council, nor any publishing committee, can afford the time and labor for research to discriminate. They must act by a mere exercise of judgment, and must act promptly, and therefore must make mistakes sometimes. By the cost of these mistakes the tendency is for them to learn to risk more on the conservative side, though this is often hard upon authors. So high an authority as Binz asserted in a paper published in the *Practitioner* that bromides were only sedative by the effect of the bases, and not by the bromine element, and it took a committee three months, making observations on something like 140 cases of epilepsy, to prove that this was an opinion and not a fact, and that it was true in no degree. More recently the statement that benzoate of sodium cured phthisis has taken a month or two of careful research and observation by able men to disprove the so-called fact. Meanwhile the benzoate men were making money at a great rate on the faith of the statement. And although it has been disproved, it will be some time yet before the unlucky doctors cease paying six dollars a pound for this benzoate.

The writer of this does not want to appear to be an apologist for a feeble *materia medica*, much less an apologist for fraud in the *materia medica*, and his past record may possibly show that he has not been especially timid in exposing and opposing both feebleness and fraud; but he was long ago taught the value of care and accuracy in making statements that were injurious or damaging to others, by being obliged to prove his facts, in view of their natural and legitimate consequences.

Now, as illustrated in the history of this case as given above, it was hazardous and unwise for the writer of the paper to make positive and definite statements of quantity from a qualitative test, which test, for aught that appears in the paper, may be, as it probably is, as fallacious as a test can be; for it is well known that the quantity or activity of a ferment cannot be judged of at all by the quantity of organic matter which carries it as a vehicle. As well might we undertake to judge of the activity of a pill from its size. It was hazardous for the writer of the paper to suggest a new test as being his own, when the maker of the article which the test and the paper lauds above all others, stated to the Society that he had used it "long before the paper was written," thus leading directly to the inference that the maker of the lauded article who read the paper, and not the writer of the paper, had really suggested the test.

Therefore it was right, proper and wise for the Council of the Society to decline to admit the paper to the pages of its Journal;—first, because it lauded one maker of pepsin and condemned fifteen others by name, upon loose and insufficient testimony, thus rendering the writer of the paper or the Society, one or the other, liable to prosecution for damages, and rendering the Society morally responsible for giving publicity, and therefore power to do harm, to loose and erroneous statements offered as facts to readers who might readily accept them as such.

Next, it was right and wise to decline it, because all the circumstances attending the presentation of the paper, when taken in connection with the circumstance that it was read by the maker of the pepsin whose article was stated in the paper to be from 3 to 38 times better than the others, led directly to the knowledge that, if published, it would at once become a most valuable business advertisement, the value of which would be based upon a plausible showing that might be very unsound.

If, under such circumstances, a council or a publishing committee, or an editor, is to be overruled, the utility of all such checks is very much damaged.

Brooklyn, March, 1880.

NOTE ON PEPSIN.

BY E. R. SQUIBB, M.D.

The foregoing paper was prepared for the March meeting of the Society, and therefore was written hurriedly; and for want of time could only treat the subject of the "Ready Method of Testing Pepsin" in a general way, which, however just and true, could be neither accurate nor definite. The failure of the Committee to report at that meeting, and the consequent postponement of the subject to this April meeting, gave the writer another month in which to go practically over the subject, in search of more accurate and definite results, in order that the "method" might have a fair trial by the common well-established test of actual artificial digestion, and the results of this investigation are to be here given.

Two stomachs from freshly slaughtered pigs and one pig's bladder were taken for the experiments.

The mucous membrane, with accidental portions of sub-mucous tissues, was dissected off from one stomach and was separately cut into fine pieces. The remainder of this stomach,—the other stomach entire or undissected,—and the bladder were also separately cut into fine pieces, and thus materials for four separate macerations were prepared. Distilled water, acidulated with hydrochloric acid in the proportion of 0.25 per cent. of dry acid, was prepared. The mucous membrane,—the remainder of that stomach,—and the bladder were each set to macerate separately in 1.963 grammes=30.300 grains, or about 67 fluid ounces of the acidulated water, and the entire stomach was set to macerate in double this quantity of the acidulated water. The mixtures were kept at common temperatures, say between 16° and 20° C.= 60° to 68° F., and were frequently stirred up. After 48 hours' maceration, and from that time with intervals of about 48 hours, for 21 days, 50 cc.= $1\frac{3}{4}$ f. $\frac{5}{8}$ of the liquid from each maceration was filtered off through paper, and was mixed with an equal volume of saturated solution of common salt, was well stirred and was allowed to stand covered for about 24 hours. At the end of this time there was in every trial an opalescent gelatinous precipitate of varying thickness found floating upon a clear solution. Occasionally, only, were there flocculi, in very small quantity, observed at the bottom of the vessels. These floating precipitates were filtered out through double filters, were well drained, and the filters were then closed upon their contents and subjected to light pressure between numerous folds of absorbent paper for an hour or two. The filters were then opened on a plate

of glass, and when the pressure had been light the precipitate could be easily and completely detached from the damp paper by a spatula, as a semi-transparent film, almost like a thick collodion film. These films varied very sensibly in quantity when thus managed, and bore no uniform relation to the measured thickness of the layers of floating precipitates from which they came. Each film or precipitate was thoroughly rubbed up in a mortar with 2 grammes= 30.86 grains of finely powdered sugar of milk, and these powders were then dried for about 4 hours at about 38° C.= 100.4° F. Each powder was then put in a labelled corked vial. The weights of these powders varied considerably and bore no observable relation to the thickness of the stratum of floating precipitate.

These powders were then tried by artificial digestion. A series of cork-stopped, wide-mouth vials of a capacity of about 37.5 cc.= $1\frac{1}{4}$ f. \AA were properly labelled for the different digestions, and a proper regulated air bath was arranged for a uniform temperature of about 38° C.= 100° F. Then coagulated white of egg was prepared by boiling eggs for fifteen minutes, separating the yolk and membrane, rejecting the soft or diffluent eggs, and pressing the white of the firmer, less watery ones, through a sieve of 30 meshes to the linear inch—mixing the product of different eggs well together, and keeping the granular mass in a stopped bottle to weigh from. Then an acidulated water was prepared containing 0.3 p. c. of HCl. or dry hydrochloric acid, with distilled water. The formula for all the trials was then adopted, of keeping the white of egg and acidulated water at constant quantities, and varying the proportion of the powders to be tried. That is, conditions of temperature, albumen, acid and water being kept constant, the digestive powder alone to vary, and the time required for digestion to be observed. The detailed formula was: Temp., 38° C.= 100° F.; white of egg, 3.25 grammes= 50 grains; acidulated water, 15 cc.= 0.5 f. \AA . And for all the powders prepared as described, from the two stomachs and the bladder, 0.13 grammes= 2 grains of each was taken, the whole being well shaken together before being put in the air bath, and every hour during the process. Five digestions of each of the stomach preparations were made, and two trials from the bladder preparation. These two, as was expected, proved that the powders made from the bladder precipitates were inactive. They had no digestive effect at all, though the precipitates were equal in volume with those from the stomachs.

From first to last the layers of floating precipitate, separated by the salt, were thinnest from mucous membrane, slightly thicker from the entire stomach, and nearly double this thickness from the remainder of the stomach after the mucous membrane had been separated, and from the bladder. All the layers became gradually thicker as the time of maceration increased up to the 20th day, and were at a maximum thickness when destructive decomposition or putrefaction was commencing; and,

as this reaction fairly started, the thickness quickly diminished. Hence, the pepsin maker who carries his macerations nearest to the point of putrefaction will get the largest volume of precipitate by the solution of common salt. But if he permits putrefaction fairly to set in, he will get a smaller volume. But he will get a precipitate from bladders quite equal in volume to that from stomachs, though it will contain no ferment.

In the digestions of the powders prepared from these layers of precipitate, those from the mucous membrane were least effective. Those from the remainder of this stomach were more than twice as effective as those from the mucous membrane, while those from the entire stomach were much the most effective, and were very active in digestion. The effect of those from the mucous membrane added to the effect of those from the remainder of the stomach, if taken together, would not equal the effect of those from the entire stomach. While the effect of those from the bladder was, as before stated, negative.

The time during the macerations at which the precipitates were most active did not seem to vary much. The observations upon this point were somewhat confused by unrecognized causes. From the 3d day to the 18th the activity did not vary much. But after the 18th day, as putrefaction sensibly approached, and as the volume of the layers increased, the digestive activity diminished, so that when the volume of the precipitate was at its maximum, the digestive activity was slightly less than it was three days before. There was, therefore, no discernible relation between the volume and the activity of the precipitate. But the precipitate might be very large with no digestive power at all,—bladder; or, might be very large with very slight digestive power,—remainder of stomach; or, might be small with slight digestive power,—mucous membrane; or, might be small with active digestive power,—entire stomach.

These results show that the precipitate is but a vehicle for the ferment, and may contain the true ferment in greater or less amount, or not at all. Hence the new "Ready Method of Testing" is not only useless, but will commonly mislead. That it did mislead the author of the "Ready Method" is proved as follows: The pepsins of five of the prominent makers of his list were subjected to his test, and were found to accord substantially with his tabular results. They were then subjected to the test by digestion and did not at all accord with his statements. Two samples, which were stated to be of very low value, were fully equal to his best samples, and all were fully up to the requirements of the labels. Therefore his test is entirely useless, except as an advertisement for the maker, [whom it wrongly extols. A table of these digestions is given herewith.

TABLE.

Showing the Results of Testing by Artificial Digestion of the Pepsins of Five of the Most Prominent Makers for the New York Market.

The digestions were made at temperatures of about $38^{\circ}\text{C.} = 100.4^{\circ}\text{F.}$ with 3.25 grammes=50 grains of boiled white of egg pressed through a sieve of 30 meshes to the linear inch, in 15 cc.=0.5 $\text{f.}\frac{3}{8}$ of distilled water containing 0.3 per cent. of dry hydrochloric acid.

No. of Sam- ple.	Test given on the label. Powder to be boiled at 100°F.	Quantity taken for the test as equivalent to 3.25 grammes=50 grs. albumen.	Time re- quired for complete solution.	Repeated digestion with smaller quantity of Pep- sin.
1 . . .	10 grains to 120 grains no time or temp. given.	4.17 grs.=0.27 gram.	3 hours.	$\frac{1}{4}$ quantity in 6 hours.
2 . . .	12 to 15 times its weight no time or temp. given.	3.33 grs.=0.216 gram.	5 hours.	$\frac{1}{3}$ quantity in 7 hours.
3 . .	10 grains to 150 to 180 grains in 4 to 6 hours.	2.8 grs.=0.181 gram.	3.5 hours.	$\frac{1}{3}$ quantity in 6 hours.
4 . . .	1 grain to 60 grains in 6 hours.	0.804 grs.=0.052 grm.	4 hours.	
5 . . .	1 grain to 60 grains in 4 to 5 hours.	0.804 grs.=0.052 grm.	6 hours.	

In this connection the want of care and want of knowledge in the great mass of the medical profession may be illustrated. The last revision of the U. S. Pharmacopœia refused to admit pepsin, and consequently there is no standard of quality or strength for it, every maker adopting his own strength and stating it roughly on his label. Of the five makers here examined, only two are alike in strength. To the digestion of 100 parts of coagulated white of egg they require by their labels, and by the actual test applied, 1.6, 1.6, 5.6, 6.6 and 8.3 parts of the pepsin. It is thus seen that the prominent makers all vary very much, the pepsins of some being from three to five times weaker than others, and yet they all sell in enormous quantities at about the same price, and physicians go on and prescribe them in about the same doses.

The difference in prime cost between a pepsin that is five times more effective than another is not so great as to make very much difference in price; and as the whole stomach used in these experiments gave the equivalent of 6 avoirdupois ounces of the strongest commercial pepsin, it would appear that the profits of the manufacture are very good. Of this strongest pepsin, 1 grain of which digests 60 grains of white of egg, the one stomach yielded 2,590 grains. This is equal to the digestion of

151.620 grains of white of egg. White of egg contains about 12.5 p. c. of dry albumen. Therefore, the 2.590 grains digests 18.952 grains, or more than 2.5 pounds of dry albumen, or 8 times that, or over 20 pounds of moist albumen, or more than double the capacity of the stomach of a living pig. Then again, the mucous membrane dissected off, and managed in the same way, did not give digestive results higher than about one-fifth of those given by the entire stomach. That is, if the ferment contained in the mucous membrane had acted during maceration upon the tissues of the whole stomach, it would have produced a solution of five times the power that it did as mucous membrane alone.

All this goes to show conclusively that the pepsins of the market are rather to be regarded as concentrated peptones, and not pepsin at all. They are the result of the action of the ferment upon the tissues of the stomach itself, and are simply in a condition so concentrated as to communicate their activity to other matters which may be in a condition to undergo similar change.

But such active peptones may as well be called pepsin as not, so long as they are able to induce the desired fermentation; and the only practical importance of regarding them as peptones is that if the process of making them be not a process of extraction, but one of true digestion, then it is better not to take the mucous membrane only, but take the entire stomachs instead,—and not to macerate them in the cold, but at the temperature proper for digestion,—and by carefully studying other conditions favorable to this particular fermentation, to obtain the most active and the most concentrated peptone that is practicable. It is altogether probable that all this has been long ago learned by the pepsin makers, but if so, the information has been kept as among their trade secrets. It is true that Mr. E. Scheffer, in his valuable paper published in the *Amer. Journal of Pharmacy* for Feb., 1872, strongly foreshadows this conclusion, but seems to regard the active peptones as mixtures of peptone and pepsin. This author, by his careful original researches, and by his frank publication of his process and results, must always be considered as the originator of this important trade interest, and his make of pepsin has always been, as it is now, among the best in the market, although not the strongest. A recent paper entitled: "Observations on Medicinal Pepsin and Artificial Digestion," by G. F. Dowdeswell, B.A. (cantab.), F.C.S., F.L.S., etc., in *The Practitioner* (English) for March, 1880, is important and timely, and some of the proportions used in the digestions of this paper are taken from it.

As a practical deduction from the facts observed in these experiments it seems doubtful whether physicians might not improve upon the common methods of using pepsin, when these are not successful, by putting the dose into the stomach with a little hydrochloric acid, and a little al-

buminous or fibrinous food half an hour or more before the meal-time, in order to produce a quantity of active peptone sufficient for the larger mass of the approaching meal. Stomachs which are deficient in the ferment are very likely to be deficient in the proper hydrochloric acid as well; and any ordinary dose of pepsin put into such a stomach after a full meal would be pretty sure to be hopelessly lost by dilution, even when other conditions might be favorable.

Brooklyn, April, 1880.

A READY METHOD OF TESTING PEPSIN.*

BY J. MERRITT, M.D.

There has been much said and written on the subject of Pepsin. Many physicians have abandoned using it on account of the frequent failure to obtain satisfactory results; in other hands, Pepsin has proved itself not only valuable, but almost indispensable. These differences may, I believe, be explained by the great variation in quality of the article, as found in the market.

The object of this paper is to suggest a ready method for estimating the amount of Pepsin present. To this I have appended the results of several observations, made by me, upon some of the best known preparations of Pepsin.

The test is an adaptation of E. Scheffer's process for manufacturing — that is in general use in this country. The process was originally described in the *American Journal of Pharmacy* for 1872. "The United States Dispensatory" (14th edition, p. 1745), under the head of "Rennet," gives the process as follows: "Mucous membrane of the pig's stomach, dissected off and finely chopped, is macerated in water acidulated with muriatic acid for several days, with frequent stirring. The strained liquid, if not clear, is clarified by allowing it to stand for twenty-four hours and decanting. The same bulk of a saturated solution of chloride of sodium is then thoroughly mixed with it. After several hours the floating Pepsin is skimmed from the surface and put on a cotton cloth to drain, and finally submitted to strong pressure to get rid of saline solution. This Pepsin, when air-dried, is very tough, parchment-like or leathery, varying in color from a dim straw yellow to a brownish yellow. To make his *saccharated Pepsin*, Mr. Scheffer adds sugar of milk until a powder is obtained, 10 grains of which will dissolve 120 grains of coagulated albumen." The "National Dispensatory" (1st edition, p. 1040) details the same process.

For the estimation of the amount of Pepsin present, put ten grains of the sample in a test tube, not less than 1 in. in diameter, add about one ounce of water, agitate frequently; when solution has occurred, add an equal amount of a saturated solution of table salt (sodic chloride); allow to stand for 24 hours; the Pepsin will be found floating in the upper part of the fluid. The quantity may be conveniently marked by a slip of

* Read at Dec., 1879, Meeting of the Society.

paper, with the name or initial of the manufacturer thereon, cut to the same depth as the floating stratum, and pasted on the tube.

Other specimens may be so treated, the results recorded in a similar manner, and thus a sufficiently accurate comparison made for all practical purposes.

This cut represents a tube so prepared as to illustrate my meaning.

Seventeen specimens are included in my observations.

All were purchased in original packages, with the exceptions of Beales' and Savory & Moore's.

The five series marked "A" were with 30 grains of the powder, in tubes $1\frac{1}{4}$ inches in diameter; in series marked "B" the tubes were $\frac{3}{4}$ of an inch in diameter; 10 grains were used of all, except No. 1 (Hawley's), 5 grains of which were employed. The larger quantity, it was found, would not rise in a tube of this size, but adhered to the sides. The figures, in these cases, have been multiplied by 2, to make them conform to the 10 grains of the others. In Series No. 6, 1 gramme of each.

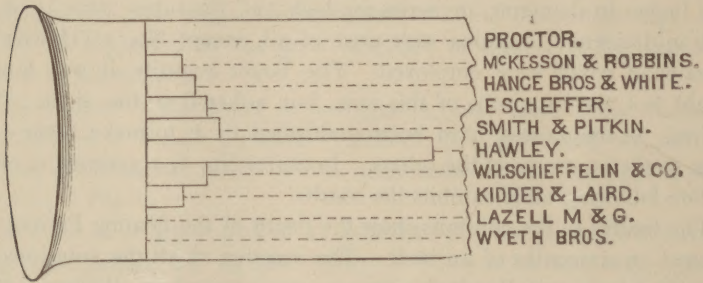
The following table exhibits the result:

The figures in the columns show the depth of the floating Pepsin, expressed in sixteenths of an inch. The reaction of all the solutions except Hawley's and Boudault's starch, was neutral or alkaline. Boudault's starch gave a ppt. after neutralization with ammonia. Boudault's sacch. and Witte's failed to give the ppt. Beales' and Savory & Moore's are the scraped mucous membrane, and contain mucus and epithelium. So much gas was generated by decomposition as to interfere with the recording of results. These five were therefore omitted after the first series. Lactopeptine was added after the sixth series. Series No. 11 was made with water acidulated with hydrochloric acid. J. Wyeth & Bro.'s new preparation, "Pepsin Porci," shows 3-16 in. for 20 grs. My supply was insufficient to include it.

NO.	NAME.	SERIES A.					SERIES B.					
		1	2	3	4	5	6	7	8	9	10	11
I.	J. S. Hawley.....	23	24	32	24	24	23	20	28	24	38	38
II.	McKesson & Robbins	1	3	3	1	2	1	0	1	0	0	1
III.	Wm. Proctor & Co.	0	..	2	1	1	0	0	0	1	0	3
IV.	W. H. Schieffelin & Co	2	5	5	6	9	11	4	6	5	4	5
V.	Lazelle, Marsh & Gardner	0	..	2	0	2	0	0	1	1	0	2
VI.	E. Scheffer.....	6	5	4	6	3	5	2	2	3	2	8
VII.	Kidder & Laird	2	3	3	2	7	5	5	4	4	3	3
VIII.	Geo. H. White	0	..	1	0	0	0	0	0	0	0	0
IX.	Hance Bros. & White.....	2	4	2	3	3	4	2	2	2	2	4
X.	Boudault (starch)	0
XI.	" (sacch.)	0
XII.	Beales	0
XIII.	John Wyeth & Bro	0	..	0	0	0	0	0	0	0	0	0
XIV.	Smith & Pitkin	4	6	5	6	7	7	6	6	4	4	8
XV.	F. Witte
XVI.	Savory & Moore..
XVII.	Lactopeptin	5	5	4	4	4	4

It will be seen that this test is qualitative, and not quantitative. To separate, dry, and weigh the precipitates, would require more attention and apparatus than the general practitioner has at his disposal. The salt solution appears to coagulate the Pepsin, and the amount is estimated by the eye, as in the case of albuminous urine.

Perhaps these facts may explain the frequent failure of preparations of the digestive principle. By this simple test the presence or absence of Pepsin may be determined.



[The foregoing paper was read by Dr. J. S. Hawley, who explained that Dr. Merritt had been prevented from attending this meeting, and had, only a few hours before, come to him and requested him to present the paper.]

